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The Author.

BLOOD-LETTING

IN

PUERPERAL ECLAMPSIA.

PATHOLOGY AND THERAPEUTICS:

THE OLD AND THE NEW.

BY

HENRY FRASER CAMPBELL, M.D.,

OF AUGUSTA, GEORGIA.

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"There is a path that leads to truth so surely, that any one who will follow it must needs reach the goal, whether his capacity be great or small. And there is one guiding rule, by which a man may always find this path, and keep himself from straying, when he has found it. This golden rule is:—Give unqualified assent to no propositions but those the truth of which is so clear and distinct that they cannot be doubted."—DESCARTES—HUXLEY.

[Reprint from August number of THE AMERICAN JOURNAL OF OBSTETRICS.]

NEW YORK:
WILLIAM WOOD & CO., 27 GREAT JONES STREET.

1876.

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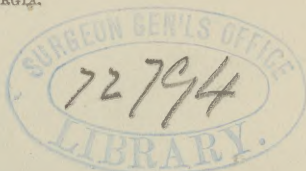
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EXPERIMENTAL PHYSIOLOGY AND THE MODERN PATHOLOGY OF ECLAMPSIA.

In the following paper we propose to examine, more or less cursorily, some of the modern views in regard to puerperal convulsions, as we find them expounded in a few of the more recent dissertations on the subject.

The convulsions of pregnancy and childbed occur under circumstances which characterize the advent of no other class of serious nervous perturbations. Concomitants dependent upon the peculiar general condition of the subject, the special condition of the blood, the state of the patient's nutrition and of her organs of elimination, serve to embarrass the explication

of phenomena which, in other forms, are most readily comprehended. It is not surprising, then, that several of these concomitants—influencing the convulsive manifestations as they often do—are not rarely mistaken for the proximate cause, or regarded as constituting the disease itself. Plethora, spanæmia, hydræmia, anæmia, besides many toxic conditions, the most prominent of which is uræmia, have each in their turn, served as the specific theoretic condition, underlying and evoking the convulsive phenomena which characterize eclampsia.

Theories, predicated the essential identity of uræmia and eclampsia, have brought puerperal convulsions into intimate etiological relation with Bright's disease of the kidney. These last relations are indeed striking and also of the very greatest importance. The general phenomena of Bright's disease, as regards the vascular system, the nervous system, the toxæmia—and often too, the aberrations in the renal secretion, as to constituents, volume and specific gravity, present themselves as a totality arising out of conditions of the body, if not identical with, certainly very analogous to, those existing in many cases of puerperal convulsions. For these reasons, it would seem unavoidable, that any course of investigation, seeking to trace the phenomena, in either case, back to their original sources, should cover ground in many respects not altogether dissimilar. And yet, the two tracks—parallel for awhile—at a certain point naturally and necessarily diverge into separate lines of both interest and research. The convulsive phenomena of Bright's disease are but the occasional evolvement from the condition of the kidney and blood-system, (capillaries and blood); while, on the other hand, the frightful convulsions of childbed—whatever may be the hidden causal basis upon which they may be founded—form, so to speak, “the head and front of the offending.”

Not always, but sufficiently often, has scientific opinion—were might say, scientific demonstration—located this etiological starting-point, in part or in whole, most plausibly in the kidney;—and sufficiently analogous are many of the attending symptoms—even outside the renal system and the albuminuria—as œdema, giddiness, blindness, sparks, flashes,¹ somnolence,

¹ From which the term *eclampsia* has originated.

&c., to render perfectly legitimate, if not unavoidable, the recognition of their very close relation. Indeed, there have been established in the minds of all, bonds which indissolubly associate them in every reasoning process, by which we would essay to study their individually complex and mutually intertangled phenomena. The widely differing prominence, however, of the two classes of phenomena, as characterizing each of the two diseases, of course now determines the investigation into distinct and separate lines. The majority of modern pathologists, while they by no means overlook the conditions of the kidney, so often aberrant during pregnancy, either in structure or function, nor the blood-system and its toxic conditions—still naturally, and we think very properly, carry their deepest researches into the penetralia of the nervous system, as the theatre in which the astounding drama—too often a tragedy—of puerperal eclampsia is enacted.¹

The renewed activity given to vivisections some time ago, (about 1850,) by Dr. Claude Bernard, and scarcely allowed to flag for a moment, by Dr. Brown-Séquard, seems in the last ten years or more, to have experienced a most decided exacerbation, in which ingenuity in devising, and cunning dexterity in performing, the operations have been sometimes perhaps the most notable characteristics of the experimental era comprehended in that period. A number of other distinguished gentlemen have also been prominently engaged, with varying degrees of success, in one way or another—whether by mechanical or electrical stimulation, by arterial ligations, or by destructive processes, in most commendable and arduous efforts to de-

¹ Among other more lengthy, and sometimes less meritorious discussions, it affords us pleasure, though differing entirely from him in many of his conclusions, to refer to the report of Prof. J. Ford Prioleau to the South Carolina Medical Association, at a recent meeting, as a comprehensive though compendious presentation of the more prominent views of certain modern pathologists and practitioners. This author reviews the earlier observations of eclampsia, not wholly neglecting any shade of opinion from Hippocrates to the present time. The doctrines of irritability, of plethora, and of the opposite state, anæmia, of the various toxic and other conditions of the blood, as uræmia, are clearly though briefly reviewed; but it is to the more recent experiments and observations upon the nerves and nerve centres that our attention is most hopefully directed, with the object of evolving the more modern ideas as to the pathology of eclampsia, as well as the guiding principles as to its treatment.

termine the endowments of various cranial nerve centres. Much of their attention has been given to the convolutions of the cerebrum; and while the observed effects of stimulation have appeared to themselves, and to some who have tested their processes, somewhat remarkable, we must say that some of their *conclusions*, if we understand them aright, appear to others even still more so. While Dupuy and Carville are said to seriously question some of these results, these multifarious experiments seem, in the minds of some, to have completely dethroned the heretofore recognized motor ganglia, as the controllers or instrumentalities of muscular activity. For an exposition of this calamitous schism, we here quote briefly from the report of Dr. Prieleau: "Especially to Dr. Marshall Hall is the profession indebted for the evidences which confirmed the theory of the diastaltic action of the nervous system, which knowledge has been the clue for the explanation of so many, and often so apparently divergent phenomena, applicable to both the healthy and the diseased condition. The carefully exact and laborious researches of such profound thinkers, skilful vivisectionists, and learned physiologists have deservedly influenced the minds of the profession. Among numerous facts relating to the nervous system, they had apparently established the localization of the centres concerned in motor energies, finding them in the masses of nerve matter, consisting of the corpora striata, the thalami optici, the crura cerebri, the pons varolii, the peduncles of the cerebellum, the lateral portions of the medulla oblongata and the anterior columns of the medulla spinalis. These portions of the nervous system have been so generally received as the seat of motor influence, and consequently the parts from which convulsive action can only [?] take its origin, that we are almost confounded to find, at this day, such assertions questioned, and that the motor forces could be engendered by any other portions of the nervous system."

Thus, it would appear, there is a tendency in the minds of some to transfer from the ganglia at the base of the brain, and from the motor tract, the seat of motor influence, so long held to reside in these parts, and to locate it in the surfaces of the convolutions. For our own part, while we are willing to admit that a series of experiments upon the cortical surface of the brain may have been valuable in evolving facts as to the endow-

ments of the convolutions generally, and also as to the specific influence of particular convolutions upon other portions of the nervous system, *as will-centres*, we see nothing whatever in any of these experiments to invalidate the results established by Marshall Hall and others, or in the least to justify the very grave conclusions apparently arrived at by the distinguished gentlemen who have been so ingeniously engaged in them. Indeed we cannot recognize any difference whatever in the phenomena evoked by most of these experiments on the convolutions, and the results of very many accidents and cerebral irritations which have been, from time immemorial, the facts of common observation, and which occur so entirely in accordance with well recognized principles of nervous physiology, that no process of reasoning is ever entered upon for their elucidation. "There is perhaps," says Dr. Marshall Hall, "not a point on the general cutaneous surface, in which tetanus, an excitomotor effect, may not originate,"¹ and the convulsive phenomena of an epileptic kind, so constantly observed to result from spiculæ of bone, from punctured fractures, from small clots, or from spots of congestion, acting as irritations on the cortical surface of the convolutions, have exactly the same mechanism as the experimental irritations; and only differ from the mechanism of tetanus, in that, in the one case, a centripetal sentient nerve is the excitor, while in the others the irritation is transmitted to the motor centres by a less elaborate and more direct process of centric reflection. The aptitude of ganglia and nerves of special sense or function to manifest their peculiar kind of activity under either artificial or morbid irritation, is familiar to every one. The eye will experience subjective flashes of light, and the ear subjective sounds, on the excitation of the nerves and ganglia endowing these organs with their respective special functions. Nothing within the range of experimental physiology to us would seem more natural or more to be expected, than that irritations of the vesicular covering or of the fibrous basis of the convolutions—be it of whatever kind it may—should be responded to by involuntary contractions in the muscles, through molecular changes propagated along the fibres connecting the convolutions with the true motor ganglia

¹ *London Lancet*, April 1857. Letter on "Excito-Secretory Function of the Nervous System."

and motor tract. This surface is the organ so far as we can locate it, of *will-force*; these fibres, the avenues of *will-transmission*; and these ganglia at the upper end of the spinal cord, are the habitual recipients and executors of *will-mandates*.¹ As the retina responds normally to rays in the evokement of luminous impressions, and the tympanum to vibrations in the evokement of sound, so does the convolution-surface of the brain, as it is thought, respond to the will in the evokement of voluntary muscular activity; and, on the other hand, as artificial stimuli can evolve subjective light and subjective sound, so in the case of the will-organ—as observation and pathology have demonstrated—when an artificial or a morbid excitation is substituted for the will, do we ever find the train of phenomena simulating, as nearly as possible, the results produced by the natural stimulus; that is, evoked *muscular action*, but *without the will*;—literally, involuntary contractions; in this case, called, “convulsions.”

No variety of opinions as to the alleged seat of the will, can materially affect our statement in regard to the interpretation of the recent experiments of Hitzig, Ferrier, Bartholow, and others, resulting in the transference of motor energy from the basal ganglia to the convolutions. It is more in accordance with what we know to be the normal result of will-action in the convolutions, to suppose that the *experimental* stimuli exciting spasmodic movements propagate molecular action along the connecting fibres to the basal ganglia in the same manner, that it is habitually propagated by the *intellectual* or *will* stimulus, in awakening these motor instrumentalities in the ordinary voluntary acts, than to admit the new theory attempted to be founded on these experiments.

And yet it is held that “this recent advancement of our knowledge of the functions of the hemispheres enlarges our views as to what may be the seat of convulsion, and furnishes us also with an insight into one of the many direct causes, by giving as evidence that an irritation applied directly to the

¹ Dr. R. B. Todd, in expressing himself on the subject of The Will, remarks, “that the cerebral convolutions with the fibres that connect them with the corpora striata and optic thalami constitute the centre of *intellectual action*.” *Cyclopædia of Anatomy and Physiology*, London, 1847. Article “Physiology of the Nervous System,” p. 723, E.

cortical portion of the brain may be as efficacious in the production of a fit, as can irritation directed to the medulla itself."

There can be no doubt that, in pregnancy and during parturition, conditions of the vesicular covering of the convolutions frequently exist whereby the convulsions are initiated, though we cannot agree that they arise from irritations transmitted *invariably* in this direction. "Metastatic labor" is a term used by one of the most philosophic of not very recent writers, Dr. Power, to indicate the discharge of the "*vis nervosa*" in other directions than in the production of contractile energy in the uterus. No one aware of the term can fail, in the observation of certain cases of puerperal convulsions, to have suggested to his mind that to change the phrase a little, a convulsion is in some respects, what might be termed a *distributed labor pain*. Few cases there are which can be regarded as apt illustrations; but, in these, the convulsion invariably returns soon after the beginning of a uterine contraction which has proved abortive; and, on the other hand, convulsion seldom takes place after the full establishment of the uterine action. There are convulsions belonging to the almost repudiated class, known formerly as those of "the hysteric kind:" and which, when occurring after labor, are almost invariably initiated by a uterine contraction or "after-pain." The purely reflex phenomena present themselves in all degrees of activity, and are not always purely convulsive. The reflected irritations may evoke sensations as well as spasmodic actions; and hence the chill as well as the jactitation; which latter we observed to occur on one occasion, after labor, with a violence and prolonged endurance, well comparable to the convulsions of true eclampsia, while the consciousness of the patient was not in the least impaired. They affected the muscles of the body *laterally*, and the alarm of the patient, a very intelligent lady, was most excessive. The after-pains were quieted by forty drops of tincture of opium per anum, and the convulsions ceased promptly on the subsidence of the uterine irritation which unquestionably evoked them. It is true, that at the present day, the interest excited by eclampsia often leads us into more recondite and therefore more attractive fields of study than those presented by the easily-read page of reflex nervous action; but may not such transition cases as the one just related, we would

humbly ask, serve as links which, if carefully studied, might complete the chain of facts necessary to the attainment of a more correct pathology?

The direct corollary from the modern interpretation of the facts of experimentation on the nervous system in relation to eclampsia, is to involve us in a discussion ending in positive denial of all former views as to depletion and repletion in the cerebral circulation. Though nearly all of the most distinguished teachers and experienced practitioners have expressly recognized the influence of plethora, congestion, and cerebral fulness, as the most common causes of convulsions, grounding upon them frequently the imperative necessity for venesection, there is no opinion now so opposed, almost with derision, as that in which convulsions are attributed to "a determination of blood to the head."

Great plausibility is apparently given to these adverse views, Many and pertinent and significant are the observations and experiments of able and distinguished investigators constantly adduced to substantiate the fact, that *excessive losses of blood produce convulsions*; and that the mode of death in extreme exsanguination is one in which convulsive symptoms are often the most prominent element of the struggle. By venesection, by arteriotomy, by compression of carotids, and by ligation—indeed, by every ingenious method in which the brain could be suddenly or slowly depleted to the extreme of anæmia, did the earnest and faithful investigators of this hopeful question energize in the elucidation of the truth. Trousseau's conclusion should have, perhaps, as weighty authority as any, even at the present time, though he did not seem to adopt entirely either depletion or repletion as the sole origin of the convulsions. "There are," says this author, "and I lay great stress on the point, two very distinct conditions in the attack of eclampsia or of epilepsy, whether idiopathic or symptomatic: 1st. A cerebro-spinal modification, unknown in its essence and its nature, which in a second abolishes all manifestation of animal life; of the two, this is by far the most important condition. 2d. A secondary congestion, which, although less important, may in some extremely rare cases be carried so far as to produce subcutaneous ecchymosis, cerebral capillary hemorrhage, and even meningeal hemorrhage."

Attractive as we might make this synopsis of modern views, we cannot occupy further space with opinions, however authoritative, so widely published as these are in books and journals at the present time. The accumulation of experiments, the variation in the vivisections, the deductive results arrived at, by the earnest investigators of the present era in this kind of research, have all tended strongly in one direction. They tend towards a pathology, the apparently accepted axiom of which is, that the cause of puerperal convulsions is ever to be found in *cerebral anæmia*; and ending in a precept of practice most legitimately deducible from that pathology, *that depletion in any form, but more particularly blood-letting*, and especially that which is applied with the view of *lessening the amount of blood in the brain, is irrational, unphilosophical and destructive*. As we have above referred to Dr. Prioleau's excellent report, and owe to it much of our apprehension of the modern views on the subject, we, in all fairness, quote his language in regard to venesection, as based upon them: "We desired to speak of the employment of the lancet. Its use will greatly depend upon the views entertained upon the etiology and the pathogeny of the fit; the time should have passed when practitioners adhering to routine and founding treatment upon the restricted field of personal experience, or upon the dicta of ancient masters who were ignorant of the physiological knowledge which we possess, employ venesection without question or discrimination in every case, as some continue to do. A personal experience is never progressive, nor is it likely to be, and is at best but a fallacious criterion of success. Our views should be more expanded. Derived from the collected information of the many recent obstetricians, each of whom is employed in the larger fields of observation and experimental knowledge; our practice should be based upon our knowledge of pathology, and upon the facts and the analogies which speculative and physiological research has contributed towards the elucidation of nervous phenomena. Speaking generally and in the most comprehensive manner—excluding exceptional instances of convulsions, which we all know occasionally occur, and which require a special treatment, an account of which we have been compelled to omit from consideration in this report—we would say, that the anæmia which generally accompanies pregnancy is the predis-

posing cause of eclampsia—the sudden arterial anæmia of the encephalon the immediate cause. Holding such strong opinions, we must be adverse to the use of the lancet.”

It is an observation not unfamiliar to those who note the progress of discovery, that neither in physics, nor in medical science, have the mere experimenters and observers of facts been always the best interpreters of their true significance. Arantius, Cæsalpinus, and Fabricius, had each, by patient labor, discovered that there were valves in the veins, which, by their inclination, marked the direction of the blood-current as plainly as the arrows of the geographer can mark on a map the course of any river. Yet Harvey, who did not discover these valves, but read more clearly their interpretation, perfected from the same facts the grandest discovery our science had known for centuries, entitling him to the appellation of “the Father of Modern Physiology.” May we not find it prudent to accept, with out some hesitating circumspection, experimental results which assume to subvert views which have so long governed treatment in this fearful disease; and which have not, to say the least, been contradicted by the teachings of experience? May we not dare to suspect, that possibly some of the interpretations reasoned out by the ingenious and skilful vivisectionists, as the pathologic significance of the phenomena they evolve, may not always answer as the indices to a true pathology, or do to trust implicitly as the infallible guides to a safe and unquestionable practice? It is the warning voice of true Philosophy which tells us that “there is no error more prolific of mischief than that of connecting observed effects with causes which did not produce them.”

Most of the deductions upon which the anæmic pathology of eclampsia has been based, have been drawn from facts arising out of conditions not altogether identical with those in which convulsions take place in pregnancy and during labor. That animals bled to death, should have, sometimes, in their death-struggle, jactitations and contortions very nearly allied, say analogous to, even say identical with, those of the epileptic seizure in the human female; or that the sudden cutting off by ligature, or otherwise, the entire blood, or as much as possible, from any considerable region of the encephalic mass, will give rise, in animals, to convulsions—we perhaps will not just now deny. These experiments are very interesting, and perhaps conclusive, so far

as *these animals* are concerned, and quite suggestive for some of the purposes of reasoning in human physiology; but we know very well that the same effect, nor anything like it, is hardly ever produced in single or even double carotid ligations in man. It has been long acknowledged that the ligation experiments of Sir Astley Cooper on dogs could not legitimately be used, as we find them educed from and expounded in the modern works, to predict results as to human ligations of the carotids; the carotids being comparatively unimportant in the cerebral blood-supply of these animals.¹ Simultaneous double

¹ "Many experiments have been made by Meyer, Jobert and others upon the lower animals, with the view of determining the effect produced on the brain by the ligature of the carotid arteries; but the deductions from these are of no value whatever when applied to the human subject; for the simple reason, which appears to have been strangely overlooked, that in many of the lower animals on which the observations were made, as the dog and rabbit for instance, the common carotid arteries are of secondary importance so far as the cerebral circulation is concerned, being destined principally for the supply of the external parts of the head, the brain deriving its chief supply from the vertebrals; whilst in other animals, as the horse, the brain derives nearly the whole of its blood from the carotids, and but a very small quantity from the vertebrals. Hence, in one case the carotids may be ligatured without danger, whilst in the other their deligation is inevitably fatal."—ERICISEN, *The Science and Art of Surgery*, p. 541.

In therapeutics, we may say, such modes of study are still more fallacious. The experiments of Laborde, Guttman, and Eulenburg with bromide of potassium are condemned by Dr. Bill, because they were made on the lower animals. "No such experiments are of any value in therapeutics. Each tribe of animals has its own peculiar therapeutics—morphia acts on cats like strychnia." The present physiology is largely, and we suppose unavoidably, based on experiments made on the lower animals; yet we constantly find such variance of action, which, like those above referred to, deeply impress us with the necessity for caution.

Though, with many others, we have long been familiar with the popular report as to the harmlessness of certain active poisons in particular animals, we desired our friend Dr. A. A. Bell, of Madison, to procure and furnish to us an authentic statement of some striking facts which had come to his knowledge:

"MADISON, GEORGIA, *July* 19th, 1875.

"DEAR DOCTOR,—Dr. R. R. Harden, of Atlanta, writes that he has himself given to to a medium-sized hound dog a *heaping teaspoonful of arsenic*, every other day for three times. It purged him some, but did nothing else but cure the mange which he had. I know it myself to be a popular remedy for mange in the dog. Have never heard of its doing any harm. The horse, sheep, and goat feed on *hemlock*. Farmers feed their young chickens on pulverized *nux vomica*, to kill the hawks.—Very respectfully yours,

"A. A. BELL.

"To Dr. Campbell, Augusta."

ligations are very apt to *kill*, but the patients die in the old-fashioned and naturally-to-be-expected way, with white softening from a rapidly-perishing brain; just as a healthy arm or leg would mortify upon the cutting off of its blood supply. They more often, we believe, die of paralysis than of convulsions. The symptoms arising from this cause, according to Erichsen, are immediate and remote—syncope, trembling, giddiness, impairment of sight and hemiplegia, are the immediate; paralysis, convulsions and death, are the remote results, marking the closing scene, after softening from impaired nutrition has taken place.

But even did we admit, for the purposes of argument, that these operations, had they been performed on man, and that whenever we bled one to death, or whenever we exsanguinated the brain of the human subject to an extreme degree, by any means whatever, epileptic or eclamptic convulsion invariably result—we could not, even then, admit the legitimacy of the comparison; for what we may venture to call the element of quantities—as brought into comparison whenever these observations and experiments are appealed to, with the view of establishing an identical, or even an analogous cause for the convulsions of child-bed—is entirely disproportionate and incongruous. The eclamptic woman can be in no condition at all approaching that of an animal which has been bled to death; for we very well know—whether the practice be right or wrong—that in the midst of this very set of phenomena, quarts of blood are often taken from the arm, certainly without always destroying life; and often with an effect, if injurious—as doubtless it often is—so inconclusive in its obvious results, as to leave on the minds of the majority of the most distinguished and experienced men in the profession, the opinion that the procedure is actually beneficial in arresting the very set of phenomena which these pathologists have recognized, as the proof most irrefragable, that cerebral *anæmia* is the cause and the condition of eclampsia! They do not all die, as we might most reasonably have supposed, but the enormous proportion of sixty-five per cent.¹ escape death, and actually get well!

On the other hand, it is argued against venesection, that ex-

¹ Leishman's System of Midwifery, p. 650.

perimental research upon living animals has shown, that the fullest degree of cerebral turgescence seldom or never gives rise to convulsions. A little reflection—simply recalling well-known principles—would have rendered elaborate vivisection on this point wholly unnecessary. Whatever may be the pathogenic influence exerted by a *moderately* increased activity or turgescence in the cerebral circulation during pregnancy, favoring *increased polarity* and *responsiveness* of the motor ganglia and of the motor tract, as well as of that immense excitor vesicular sheet spread over the convolutions, commonly known as the cortical substance, we will not here attempt to estimate. We are free to say, however, that it is *increased polarity and increased responsiveness to near, or to distant excitor impressions*, that we regard as the condition essential to the occurrence of convulsions during pregnancy and child-bed;—that it is indeed the eclamptic state.

We have regarded it as too long a settled fact in pathology, that cerebral blood-pressure, or any *other kind* of equable cerebral pressure, could ever have such an effect, to doubt for a moment that such a state of pressure is the very last condition that could give rise to convulsions. Such pressures upon the encephalon, by common consent and common observation, are attended by an exactly opposite condition—that of paralysis; for there are apoplectic states of the brain which are the *ipse morbus* of certain forms of paralysis.¹ Such turgescence, such congestion, blood-fulness, serum-fulness—essentially such pressure upon the brain, and not “exhaustion of nerve-force,” as often suggested, is *the one grand essential* in *arresting the*

¹ Long since, the influence of blood-pressure upon the brain had been carefully studied out, and as forcibly demonstrated as logic and language could disclose a physiological process. It was unquestionably proven that pressure of any kind, whether by blood or depressed bone, by pus, by serum, by distended vessels, or by extravasated clot, instead of giving rise to convulsions, invariably produce the opposite—paralysis. And yet vivisections and ligations are being diligently wrought to demonstrate this principle many years proven; viz., that extreme cerebral congestion does not produce convulsions. These repetitions are unprofitable. Is there no tangent at which physiologists may cast themselves out of this charmed circle in which they are performing so tiresome a round, and in which pathologists and practitioners must necessarily follow, as travelers guided by their pioneer? Science may be puzzled and embarrassed by re-opening settled questions in physiology, but it can never be advanced.

convulsion, and in obtunding, as opium does, or as chloroform may for a moment or two—the reflex irritability of the cortical surface, of the motor ganglia, of the motor tract, of the cutaneous surface, of the uterine nerves, and of the sensori-motor apparatus universally; which reflex irritability, from whatever source it may have taken its origin, whether from an irritative hyperæmic condition of the brain itself, or of the sensory or motor ganglia at its base, or from too much urea in the blood, we here repeat, is the one ultimate and essential cause of most puerperal convulsions. A moderate hyperæmia of the eye renders the retina exquisitely intolerant of light, convulsing the iris as well as the associate *motores oculorum*. Advance this to actual apoplectic congestion, the retina becomes irresponsive, and the reflex motor phenomena entirely disappear. It is well-known that active leeching, or opiates, would, in most cases, promptly have relieved this irritation. The parallel is obvious.

Of course we are fully aware that it will quickly be asked: If you regard “determination of the blood to the head” (and we have no objection to the honest old phrase) so essential, in a certain way, to the arrest of the convulsions, why is the drift of your argument such, that we recognize in you an advocate of the lancet? We answer that we are not so much at present the advocate of the lancet, as that we are asking to be recognized as the defender of the use that has been made of it in the past. We do not claim for it that indispensable value which, not very long since, we insist, was most justly accorded to it. We do not regard it *now* as the “sheet anchor”¹ in the treatment of eclampsia; but at the same time we do not wish, that its non-use and abandonment, as a sole reliance, by ourselves and by others, shall be regarded as the substantiation of doctrines in physiology and in pathology, which we consider as by no means legitimate; and which are gaining triumphant though undeserved practical vindication, almost solely, by the entirely groundless and unwarrantable interpretation of blood-letting in eclampsia, as it has been extensively practised in the past, and by, here and there, a few in the present.

We wish to break, in some degree, the force of that spurious argument which makes it a crime of ignorance ever to use it;

¹ Gooch on “Diseases of Women.”

and, if possible, to call attention to some of the fallacies which attach both to the observation of some of the experimental facts, as well as most especially to the momentous deductions evolved from them in regard to a remedy which, for centuries, under the most careful—we may say, the most jealous observation of the best medical philosophers, continued to be recognized as the accepted and most reliable resource then possessed for the control of eclampsia.

Blood-letting has ever been so decided and striking a measure, that when, in the progress and advance of therapeutics, other remedies of less exhausting influence, but possessed of sedative powers capable of controlling convulsions, began to come into use; each one of these was set up, not as an adjunct to, or as an improvement on, the good and well-tested reliance of so many years—not as a some-time substitute, not as an improvement simply—that is, a *better* method in the place of one acknowledged to have been *good*—but these remedies were brought forward as rivals and antagonists; uprooters of that which had so long been the recipient of confidence and favor. A new *modus operandi* was claimed for the new remedies which was entirely adverse to that of venesection, and which, if established, would prove that the practice of blood-letting was then, and had ever been, entirely ignorant and injurious. Venesection, at one time almost our only remedy, had been used so long and so freely and so generally, that, with all the praiseworthy attributes it had gathered, it had accumulated also, many sins. Among the thousands its judicious and proper application had saved from death, there were doubtless also many, who, by its improper employment, had been killed. The “new issue” of physiologists, pathologists and practitioners, began to condemn it with a wholesale condemnation. So far from seeking its many merits to disclose, they with ruthless and unappeasable rigor, drew all its frailties from their dread abode. The practice is now not only condemned as being inferior to other sedative methods of quieting convulsive action—as we freely admit it often is—but it is even insisted that it is not a sedative at all. It is not only argued that it is unnecessary to use it, now that we have equally good and less exhausting remedies, but that it never had been right to use it, no matter how destitute we may have been of means by which to quiet convulsions;—not only that its use is

never now right, but that it had always been totally and entirely wrong. And more than this; with the refrain of an old chime constantly repeating itself in their minds,—*atonie gignit spasmos*—they enter what is thought to be the decisive field of vivisections and of mechanical and electrical irritations;—they overturn and overturn, until they have established in some sort, a physiological basis for their condemnation of the lancet, which is so logical and so impregnable, that those who, at first, thought to question it, at once succumbed.

We now look with horror upon the ignorance of our forefathers; and in shuddering wonder, we thank Heaven for the sixty five miracles which must have been enacted wherever any hundred pregnant or lying-in women had been subjected to blood-letting in eclampsia!

But their vivisections and the generalizations from them prove too much for common observation and for common reason to accept:—they lead unavoidably to conclusions, which, if extended, may presume a most frightful condition of anæmia, some forms of which may, even of themselves, “suddenly extinguish life.” In this state of extreme exsanguination, could the brain, from any influence whatever, bear the farther diminution of the amount of blood upon this uttermost minimum to which it is already reduced? We are compelled to agree, if we accept their rationale, with the very extremest of those who radicate the convulsion in anæmia, that most assuredly it could not. Upon such strong deductions—all based upon physiological experiments—has the impregnable doctrine of the anæmia pathology been founded, and by these deductions has it practically gained an almost universal support at the present time.

But, as we have said, these experiments and generalizations in proving such extreme degrees of cerebral anæmia as the common condition of the brain in eclampsia, have proved too much; and we might add it has been promulgated an entire age too soon. There are too many practitioners yet living who have witnessed the escape of too many women (said now to have been the subjects of such anæmic brains) after profuse and repeated bleedings. The sweeping condemnation of the once time-honored measure, while it saps the logical integrity of the reasoning of many we honor and revere among the dead; also, most unfortunately for itself, aims a mortal blow

at the justness of the life-long convictions, and labors founded on them, of some few who are yet living. Some few who can remember, though perhaps they cannot reason;—who can count, though they may not be able to philosophize; and who can compare even though they would not presume to generalize. There are men now living, who still remember their own successes in that most destitute past, when only the lancet and a few minor adjuncts constituted their entire fund of resources even in the most frightful cases of eclampsia. With that “meekness of wisdom,” so characteristic of an enlightened advance in professional life, they hailed with cordial pleasure the achievements of later investigators. They have modified their method—they accepted improvements, bleeding less and trusting more, year by year, to anæsthetics; till, while they remember with gratitude the many mercies often accorded them out of the very depths of despair, through the direct and unquestionable instrumentality of the lancet, they have yet accepted the new and less exhausting remedies. But they have adopted them strictly as substitutes—only as less exhausting substitutes—for that which once had served them well, and which now, perhaps, they lay most tenderly and reverentially away. These venerable men—and some of them are not yet even gray—as we have said, can count, and they do count the number of escapes that were made by patients, who have been treated on the once successful, but now condemned and reprobated plan. They candidly acknowledge, too, that there is a difference in favor of the substitutes. They study the statistics which were expected to stultify themselves, and they magnanimously regret to find that the difference is so little. They examine the pathology which has guided to the principles of the new practice, and they are filled with wonder when they realize how little the noon has gained upon the dawn of science!

These men are by no means amenable to the charge of unprogressiveness in science. Though out of the sphere of the latter-day progress in vivisections and in experimental physiology, they yet look with hope and pleasure to this as the source whence is to come the confirmation of many of their own results of observation, as well as for the clearing of obscurities they could not in their own day penetrate. But when, instead of this, they find these experiments are to become the pro-

crustean bed upon which are to be laid established truths as well as possible errors ;—when long demonstrated facts, as the functions of the brain-centres, are to be denied and uprooted, because they do not quadrate with modern interpretation of modern experimentation ;—when absurdity appears in the main induction of this modern reasoning—they may, for a time, be forced to silence ; but there they will still remain ; and while admitting the converse, per force, they rise stronger than ever, with *e pur si muove* as their final and indignant protest.

For vivisections, for all experimental demonstrations of physiological and pathological truth, we have the profoundest and most ineradicable reverence ; but when the deductions evolved out of them lead to absurdity, we take refuge with the more reliable and far safer guides which are ever to be found in the teachings of experience and in the intuitions of common sense.

The idea of cerebral and centric anæmia seems every day to be extending its applications, and sometimes too, in accounting most strangely for very opposite kinds of phenomena of disturbed and morbid action. Not only are the jactitations initiating syncope and the convulsive struggles attending death in the shambles appealed to, in order to confirm the doctrine that all convulsions depend upon centric exsanguination ; but now we find so high an authority as Niemeyer contending, that even the paralysis resulting from cerebral hemorrhage (apoplexy) is not due directly to the pressure of the clot, but that “it is attributable to the sudden compression of the capillaries producing anæmia of the brain substance.” In answer to this, we might adduce the fact that, unless he explains paralysis from depressed bone in the same way, his rationale will not hold good. Again, Professor Bauduy, of Missouri, remarks that “some maintain that reflex paralysis is induced by an anæmia of the blood-vessels in that particular portion of the cord where the irritation is reflected ; hence, they call it a reflex spinal ischæmia.”¹

Thus we see how various are the phenomena attributed by the more modern writers to the instrumentality of the brain and its basal ganglia, when they may have been from any cause put in

¹ Lectures on Diseases of the Nervous System.—Philadelphia, 1876. p. 334.

a condition of lessened vascularity. Some attribute convulsions to this condition; while others again, as just quoted, explain the diametrically opposite phenomena of paralysis ~~to~~ this same condition of anæmia. Animals that have undergone carotid ligation, animals that have been bled to death—persons in syncope, persons suffering from cerebral hemorrhage—all these various subjects are assumed, and probably correctly, to have anæmic brains and anæmic basal ganglia. According to some, these exsanguinated centres *convulse* the muscles; according to others they *paralyze* them.

While we would not wish to raise any question which might unfairly disconcert the varied applications of this very accommodating theory of anæmic nerve-centres, there does occur to us one other familiar set of well-understood convulsive phenomena, in which it would surely seem that neither the anæmic brain nor the anæmic basal ganglia can exercise the least energetic instrumentality in the production of convulsions:—We refer to the very marked muscular jactitations following upon *complete decapitation*. We know full well, as every one else does, how Dr. Marshall Hall would most consistently interpret these contortions according to the principles of his “true spinal system;” but should not their occurrence—the head being entirely severed—lessen our confidence in the so-called cerebral anæmia, relied upon as the cause of puerperal convulsions?

ANIMALS BLED TO DEATH DO NOT DIE IN CONVULSIONS.

Having doubted for some time the legitimacy as well as the cogency of the arguments, so long and so often attributed to analogies drawn from the convulsions of slaughtered animals, we, during the preparation of this paper, began to cast about for some striking and convenient experiment which might test the value of the very strong assertions based upon what appeared to us rather imperfect, if not unintentionally perverted, observations. The most common and familiar example of such convulsions is that of the killing of the ordinary domestic fowl. In this case, however, the mind being impressed with the common idea that death by hemorrhagic exsanguination is always necessarily convulsive death, it was difficult to decide whether to the severing of the arteries, or to the simultaneous

severing of the cervical cord, was to be attributed most of the very marked contortions observed in such killing; for, in the majority of such instances, complete decapitation is the method adopted. There was reason, however, to believe that *bleeding* was *not* the cause of these same contortions, which, as we had often observed, are invariably produced by simply crushing or wringing the neck, without separating the head at all, or producing any effusion.

With this view, we determined to modify the mode of producing death, though we here distinctly reiterate our opinion as to the *non-conclusiveness* of such experiments on the lower animals, as illustrative of the effect of morbid cerebral conditions in the human female during the eclamptic seizure.

In our first experiment the subject was a full-grown and vigorous young fowl. We severed at one incision all the vessels, nerves and other tissues of the neck in front of the vertebral column, leaving uncut only the trachea. The blood was rapidly poured out. The fowl became weak and lay quietly upon its side, making no convulsive movement, and manifesting no disposition thereto until touched, when the actions were evidently of a voluntary character and produced by fear. Not being touched or approached again, there were no movements for a considerable time. Gasping soon succeeded; then an opisthotonic condition of the lower limbs and body; and lastly, a quivering and tremulous movement of the wings and wing feathers, not fluttering as in the succeeding case. The bird now on being taken up was found to be dead.

The subject of our second experiment was also a vigorous full-grown fowl. The feathers being removed from the back of the neck, we severed, with a delicate bistoury, the spinal cord about the middle of the cervical region, passing the knife carefully between the vertebrae. But a few drops of blood were lost. The most violent convulsive actions, jumping from the ground, fluttering with the wings, and indeed all those spasmodic movements familiar to every one who has witnessed complete decapitation, were the instantaneous result upon the cutting of the cord. These lasted some fifteen or twenty minutes, becoming somewhat less marked. Decapitation was now completed. After this, the convulsions continued a considerable time and then ceased. These spasmodic actions certainly

did not increase on the pouring out of the blood by the severing of the cervical vessels.

The difference in the effect of decapitation on various animals, if systematically observed, would be most striking. The widest difference, perhaps, would be found to exist between birds and reptiles: while the muscular contortions of decapitated birds are of the most violent and irregular character comprehending energetic movements over a considerable space the moment that section has nullified the inhibitory or co-ordinating influence of the encephalon; the movements of decapitated reptiles, as the saurians and chelonians, are entirely different, being restricted as to their scope, and of a character very little differing, in precision and co-ordination, from true voluntary muscular acts. This we exemplified by the decapitation of two fresh-water tortoises shortly after the experiments above described; in these there were no movements that partook at all of the character of convulsions. The fore-feet were pretty constantly drawn beneath the carapace, but the hind-limbs frequently executed movements outward and backward, sometimes touching the ground, lifting up the body, and in every respect closely resembling the motions exercised in walking. These seemed entirely independent of any response to reflex irritations caused by the touching or the pricking of the foot.

Dr. Bennet Dowler, of New Orleans, was engaged many years ago in some interesting studies on the nervous system of saurian reptiles, which are still familiar to many. The subject of his original and ingenious experiments was the great amphibian of the south-west; and his object was, the investigation of "the seat of volition in the alligator." His results were of the most striking character, and demonstrated, that after removal of the cerebral masses, co-ordination under reflex irritations was so complete, and muscular movements so well concerted and apparently intentional, as to leave the question unsettled whether or not volition in these reptiles was an attribute of any of the constituents of the encephalon, or of some other portion of the nervous system. Such wide variance in the effects produced in the different orders of animals by removals and separations of the cerebral ganglia from their connection with the spinal centres, the true seat of convulsive action, should cer-

tainly lessen still more our confidence in all inductions relating to the human subject drawn from such analogies.

In conclusion, we would say, we have been much surprised at the importance sometimes attached by certain physiologists to the struggles of animals dying in the slaughter-house, in their attempts to elucidate delicate questions of pathology; for it must be remembered, that in most of these modes of death the cause producing it is often of a composite character. The animal is first "knocked on the head" to disable it, and then "stuck" to exsanguinate or "whiten the flesh." That under these circumstances there should be convulsions, is not at all surprising; but that careful physiologists and pathologists should attribute them in such a case to one only of the combined causes attending the death, and upon this assumption found principles for the decisions of practice in circumstances of dire and consummate extremity, must appear to any one as at least in some degree a failure in circumspection. If such slaughtered animals really die in convulsions, who can legitimately decide whether the crushing blow upon the head or emptying the vascular system of its blood, is the true and only cause producing them? We have seen that the domestic fowl at least can bleed to death, and yet no convulsive struggle characterize the cessation of life.

PUERPERAL WOMEN BLEEDING TO DEATH DO NOT DIE IN CONVULSIONS.

But let us confine our thoughts to the very subjects themselves—to the pregnant women impressed with all the conditions that coexist with eclampsia.—How is it with them when they suffer extreme exsanguination? Do *they* die in convulsions when this frightful casualty befalls them—when they bleed to death? The answer from every experienced practitioner would be a most decided *negative*. Fortunately the calamitous occurrence of actual death from uterine hemorrhage, whether ante- or post-partum, is an extremely rare one, since the modern hemostatic resources are so fully and so readily at hand;¹

¹ "Perchloride of Iron, in Hemorrhage after Labor." By Robert Barnes, M.D., &c., London. Vide "Obstetric Operations." Second American Edition; Chap. xxviii., pp. 449-458.

Although not directly connected with the present subject, we take occa-

yet nearly every one has witnessed the extreme exhaustion attending placenta prævia and other distressing conditions; and yet in the report of none of these cases do we find that eclampsia ever comes to close the scene. *Convulsions* are never thought of, even though "anæmia and the exsanguinated basal ganglia" must unavoidably occur to all philosophic minds who contemplate such conditions. Let us here close pertinently this much of our subject by a note from the *Transactions of the New York Obstetrical Society*: "Dr. Jacobi agreed with Dr. Thomas, that it is better to defer operating when the patient has become debilitated from repeated hemorrhages in placenta prævia, than to run the risk of the shock which an operation might produce. As to the mode of death, does the patient die from shock, or from anæmia of the already exsanguinated brain and medulla oblongata, unavoidably following the rapid evacuation of the uterine cavity by version and extraction? He is confident that the latter is the true cause. To this Dr. Thomas assented."¹ Can anything be found more pertinent to our subject—can anything more clearly *contradict* the anæmic theory of puerperal convulsions?

Thus have we shown, as it were, by gradual approach, the convicted fallacy of some of the long-admitted arguments upon which it has been assumed that puerperal eclampsia is the

sion to refer with much commendation to the paper of Dr. James Trask, of Astoria, N. Y., on "Injections of Tincture of Iodine into the Cavity of the Uterus in Hemorrhage after Delivery."—*American Journal of Obstetrics*, Feb. 1875, p. 613.

That the use of solutions of perchloride of iron, as suggested by Dr. Barnes, of London, has been instrumental in saving many lives, is most unquestionable. Its occasional failure and still less frequent disastrous results have not deterred the profession from using it, *as a last resort*, in which case only it seems to have been the intention of Dr. Barnes to recommend it. We have now, however, in the reported experience and researches of Dr. Trask, a most efficient and innocuous styptic which, while it surely excites contraction of the womb and uterine blood-vessels, thus promptly arresting the hemorrhage, yet never excites inflammation nor leaves any coagula, to produce irritation or by absorption, to lay the foundation of septicæmia. His formula is: "Tincture of Iodine, ℥ ss, water ℥ i. Inject into the womb." It is antiseptic as well as styptic. Dr. Trask's paper presents a valuable and scrupulously fair *summary* of the discussions relating to styptic methods, in post-partum hemorrhage.

¹ *Transactions of the New York Obstetrical Society*.—Reported by Paul F. Mundé, M.D., Secretary; *Am. Jour. Obst.*, Feb. 1876, p. 650.

result of cerebral anaemia:—First, that animals bled to death do not die of convulsions unless the cervical cord is severed or injured, or some injury done to other nerve centres; and secondly, that pregnant women, in bleeding to death, *do not have convulsions*, even when “the exsanguinated brain and medulla oblongata” are distinctly recognised “as the cause of death.” This is an observation upon which we could not well improve—an argument made so fitly to our hands that we must not weaken it by a single word.

MODERN PATHOLOGY AND THE THERAPEUTICS OF ECLAMPSIA.

When the assumed condition of the brain in eclampsia—that of extreme anaemia—is considered; that sixty-five out of every hundred subjected to venesection should escape; it ought to be regarded far more wonderful—according to the reasoning of this pathology—than that eighty-nine should be cured by any other method of treatment. Blood was taken out of the body and out of the brain—in some of them largely taken—in order to cure the convulsion, the practitioner being ignorant of the fact that these very convulsions had their origin in an already deficient quantity of blood in the brain. The treatment had been predicated upon plethora of the cerebral blood-vessels, when in reality exactly the opposite condition existed—anaemia!

When to the experienced practitioner we say, there are some few morbid conditions for which we prescribe, in which all the elements of diagnosis, so to speak, can scarcely be said to have been supplied until the results of treatment have demonstrated the correctness of the assumed pathology, he will fully comprehend the remark. Let us see if the anæmic pathology of eclampsia, so confidently assumed, and based upon vivisections and the deductions of vivisectionists,—who, as we have humbly ventured to suggest, may not always reason so well as they dissect—let us see if this extreme exsanguination of brain is substantiated in the way referred to, by the results of treatment. Does the effect produced by remedies prove, or does it disprove, the existence of that condition of brain, which, to these pathologists, is the *ipse morbus* of eclampsia—the convulsions being but its most prominent manifestation? In regard to the effect of venesection—the remedy of the past—it has been seen how

from the very first moment that we began to consider the subject of cerebral anæmia, down to the last word we could say about it, we but struggle upon the horns of a most perplexing dilemma. If the eclampsia of the past was extreme cerebral anæmia, then not one in a thousand who were bled should have escaped death; and yet statistics prove, that sixty-five in every hundred got well under bleeding! And with an equal confidence do we state, almost in the same words, the parallel proposition, that if the eclampsia of the present is extreme cerebral anæmia, not one in thousand, who are now subjected to chloroform, chloral and the bromides should be expected to escape; and yet the statistics, admitted on both sides, prove that eighty-nine in every hundred do get well under anæsthetics.

The double dilemma to which we have thus we think very fairly been brought, by subjecting the anæmic pathology to examination, under the lens of a therapeutic as well as of a pathologic illumination, is truly disconcerting; for in this awkwardness of discussion, we have fallen into the danger of unsettling our claim to that exact knowledge of the nature of the disease and of the rationale of treatment, of which we had supposed we were in such quiet possession. We do not see, in the turn which the discourse has taken, how we can escape from the entanglement into which we have so heedlessly fallen, except by a sacrifice perhaps of the gravest kind: Our pathology or our therapeutics must, one or the other, inevitably succumb. Had all the eclamptic women in the past died from bleeding whenever it was employed, or had all the eclamptic women in the present died from the use of those powerful depletants of the cerebral circulatory system—the bromides and chloroform—how triumphantly would our experimentally established physiology and our rationally deduced pathology have been vindicated. Or if the remedies which cured them had been brandy and strychnine and electricity, we could still claim the infallibility which we have asserted—but chloroform, the bromides, cerebral exsanguinators! How can *they* arrest convulsions eventuating from an “exsanguinated brain and medulla oblongata?” Verily, has Hahnemann at last been vindicated?—*Similia similibus curantur!*

We see no possible way out of the prison-house now closing around us. Perhaps we may impugn the theory which alleges

the therapeutic action of the bromides to be that of cerebral depletants. The inevitable words are, "they lessen the amount of blood in the brain,"¹ and in this unquestioning faith, we rely upon them to cure grave diseases for which, in a former age, we medical men have been known to tie both of the common carotids; and they sometimes cure these diseases, when tying the carotids used often to kill. It is our principal remedy for epilepsy. We would fail at present in any attempt of this kind; the bromides hold the title to this alleged attribute from authorities fully as high as any that have asserted "that which is the essence of anæmic convulsions." And then there is chloroform, one of the very chiefest of all the anti-convulsives; one which, according to such unassailable authorities as Dr. Nélaton of Paris, and Dr. Sims of New York, kills, whenever it does kill, by producing cerebral exsanguination to such a degree and so quickly, that "the anæmia at once destroys life"—this assertion being further proved, in that by reversing the blood-statics, the blood by gravity is "determined again to the head," and the patient is recovered. We cannot revise the entire pharmacopœia even to save our doctrine. Indeed, too many things cure eclampsia. Quinine is said to act favorably, and that too is "a contractor of the middle coat of the blood-vessels,"² and a repeller of the blood from out of the brain. Even opium, in some of its forms, is thought by some to do good; but that also "contracts the arteries" and lessens the amount of blood in the brain, as well as in some other portions of the system.³ But after all—even

¹ Though we do not question for a moment the truth of this rationale of the action of the Bromides and of several of the other anesthetics above mentioned, we also recognize as correct the views in regard to their action expressed by Dr. Bill, (*Amer. Jour. Med. Sci.* July, 1868), viz.: that "*they are anesthetics to the nerves of the mucous membranes.*" We have no doubt whatever, that much of their wonderful potency in arresting and preventing convulsions resides in this last attribute. Where the irritation is peripheral and the convulsions reflex, as in infantile convulsions or eclampsia from uterine irritation, the excito-motory action is arrested in its inception. We use the bromides largely in cholera-infantum to obtund the morbid sensibility of the mucous surfaces, which gives rise to the excito-secretory or so called "vaso-motor" action upon which, during dentition especially, this often unmanageable affection most frequently depends.

² The Rationale of the Action of Quinine, by Dr. Robert Campbell, then Demonstrator of Anatomy in Med. Coll., Ga., Augusta, will be found fully stated in *Southern Med. & Surg. Jour.*; Vol. xv. 1859, p. 569.

³ Treatment of Ulcers; by Dr. F. C. Skey, of London.

succeeding in correcting the false views in regard to the *modus operandi* of these remedies—what can we do with venesection itself? Even that did not kill as it ought to have killed. What can we do with sixty-five in a hundred who would persist in getting well notwithstanding the anæmic source whence the convulsion came, and most perversely, in spite of *atonin gignit spasmus*!—Not even for bleeding do they die near fast enough to vindicate the claims of science. The dilemma, it is said, ever forces a conclusion. Let us see—though not after the strictest method, perhaps:—notwithstanding the fact that we have not disproved anæmia of the brain and its ganglia during the time the eclamptic convulsions are in progress, it would plainly be highly unphilosophical to conclude that such existing anæmia could possibly act as the proximate cause of the convulsions, when the very remedies admitted on all hands to be the most effective controllers of such convulsions are also the very agents which are, by all our experience, pre-eminently known to be such, as in their action produce and increase anæmia of the brain. We may conclude, then, that while anæmia to a considerable extent may sometimes exist at the time of the convulsions; and, if so, be increased by the use of the remedies which are applied to relieve them; still, inasmuch as such agents do relieve the convulsions, even though increasing the anæmia, some other cause than the anæmia must have been present to produce them; and consequently it must have been *this other cause* that was successfully combatted, and not the anæmia, when the convulsions were relieved.

In a former period in the history of medical reasoning, not very far back, it was attempted to found pathology upon the basis of physiology, and therapeutics, most strictly, upon the basis of the pathology thus deduced. Who can find fault with so orthodox a construction of the organic law of medicine? The formula was admired, approved and pretty widely accepted; but when examined, strange as it may appear, this commendable and what would at first sight appear, this only legitimate project of construction, had virtually to be abandoned. The first link was defective, and every portion of the chain gave way. Physiology, the foundation upon which the entire fabric was to have been reared was found to be imperfect and unreliable—not physiological *truth*, but physiological *deduc-*

tions and doctrines—hence the failure. Of course this interdependence is still the directing and controlling influence in all true progress, but only by slow advances and in restricted lines. There are many wide gaps and dim paths and dark places, through which pathological reasoning cannot guide us. Here personal experience, observation and phenomenal light—empiricism—must govern and direct us. Notwithstanding all that Marshall Hall, and Claude Bernard, and Brown-Séquard; and Hitzig and Ferrier, and Shroeder van der Kolk; and Radcliffe, Kussmaul and Tenner; and Dalton, Flint and Nothnagel; Fritsch and Jackson and Fournier, and many others, may have accomplished in regard to the nervous phenomena concerned in, or that can be applied to, eclampsia—notwithstanding what Hamilton, and Demarec, and Lyman, and Bright and Simpson, and Lever and Regnault; and Dubois and Cazeaux and Blot; and Litzman and Braun, and Segbert and Corel, and Ranvier and Barker may have accomplished in the toxæmic relations of the subject; and notwithstanding, still, the valuable labors and diligent researches and generalizations, such as we are now discussing,—notwithstanding all that has been done directly or incidentally to illumine the darkness and to dispel the uncertainty, still we dare not even now, deduce the pathology of eclampsia from the physiology which, from vivisections and other experimental methods of research, has been most recently promulgated in this and other countries. And on the other hand, even still less could we *begin to dare* to adjust our therapeutics to the teachings of that pathology, or to the rationale evolved out of the deductions of experimental physiology during the last decade of our own era.

We cannot then regard the pathology of eclampsia to have been very materially advanced, either in definiteness or in extent, by any of the recent experimental researches or the deductions from them, that we have thus hastily though we hope fairly examined.

In regard to methods of treatment for eclampsia, practice fortunately has not always been governed by the suggestions of the prevailing physiological, or again, even of the pathological views of the disease. For years, the doctrines of uræmia, and also of anæmia, have been predominant; and yet, as we have seen, whether forgetting the pathology they professed to hold,

or forgetting the *modus operandi* of the medicines they actually applied, the very men who would advocate the anæmic pathology, and—as an obvious corollary, abhor bleeding—seem all this time, with common-sense empiricism, to have been giving chloroform, the bromides, etc., in the very face of that obvious logical necessity, fore-dooming the patient on the still further lessening of the blood in the brain. Can we venture the opinion, in explanation of this incongruity, that the practitioners were not physiologists any more than were the physiologists, especially in such cases, ever practitioners; or did they know the wrong and yet the right pursue? In any event, how fortunate was it for the imperilled patient!

But it was not every one that pursued his practice in eclampsia on a method so loose of purpose as that he would violate pathological precepts and guides, albeit in doing so he cured the patient, without recognizing the breach. Even long since the prevalence of the doctrines recognizing anæmia, and also uræmia, as the pathological basis of the assemblage of frightful nervous phenomena characterizing the eclamptic condition, every now and then do we observe certain thoroughly philosophic, but independent practitioners, who, while they find no ground to deny the truth of these doctrines, still could not shut their eyes to the fact, that their own experiential knowledge and observation, as well as the statistics of practice, at least vindicated even venesection from that charge of danger and of fatality so often brought against it. Prof. B. W. Richardson, of London, is one of this class. Driven, like the rest of the profession, from the position that plethora, or any other demonstrable circulatory aberration, could be recognized—since the views of Karl Braun, at that time everywhere accepted—in the etiology of eclampsia, he seemed disposed to admit the presence of an irritant in the blood, as the cause inciting to the convulsions. He did not at the time deny that urea might be that irritant. Notwithstanding all this, Dr. Richardson too clearly understood, both from his own observation and from the history of venesection in eclampsia, that it was by no means to be relinquished. Its benefits could not be denied; so he set patiently to work philosophically to explain them. Hence we have the reconciling explanation peculiar to himself, and doubtless in many cases a fair expression of the truth, that if

he must relinquish plethora as the cause of convulsions, and no longer bleed for that, as uræmia is now the cause, *he still would bleed* in order to secure a rapid elimination of poisonous blood, uræic blood, from the system.¹ This was indeed very courteously letting theory have some of the benefits and credit of practice, without in the least stultifying either his observation or the experiential facts of history.

Prof. S. D. Gross,² in most decided, if not irate terms, reviews and condemns the crusade widely entered upon against bloodletting. We regard his impatience as by no means unrighteous,—but we quote first some of his sage precepts, and then his running comments in deploring its abandonment. “In puerperal convulsions, attended with a plethoric condition of the system,” he recommends “copious venesection, promptly followed by the administration of a full anodyne, either alone or in union with chloral and bromide of ammonium, and the application of leeches to the temples and cold to the head.” “The plethoric condition of the system, so frequently met with in young, robust, pregnant women, is generally promptly relieved by the abstraction of twelve or fifteen ounces of blood; and certainly there is no more rational remedy in such circumstances, especially when redundancy of blood is accompanied by dizziness, vertigo, or headache. Thirty years ago there were but few women who were not bled once or twice during utero-gestation, on account of these symptoms, and I do not know that I ever heard of one that was injured by the practice.” “Certain forms of hysteria and epileptic convulsions, dependent upon congestion of the nervous centres,

¹ The removal of a mass of “poisonous blood out of the system” is not, we conceive, the true explanation of the benefit of bleeding in uræmic puerperal convulsions. It does far more than this; it relieves the engorgement of the kidney and rapidly restores its secretory function, and thus the entire system is purified by its natural depurater. Whatever may be our ideas as to cerebral plethora or cerebral anæmia, bleeding for renal plethora is a necessity unavoidable and imperative; for no other diuretic can act, till the kidney is relieved by bleeding of its paralyzing engorgement. The older practitioners bled women to relieve them of the dropsy of pregnancy. They knew nothing about uræmia, but none the less did they clear it from the blood when they, by venesection, started the kidneys to drain the redundant water from the tissues.

² Discourse on Blood-letting considered as a Therapeutic Agent. *Transactions Amer. Med. Association*. Vol. xxvi, p. 421. 1875.

and a redundancy of blood in the system, are generally materially benefited by venesection. The relief in the former of these affections is often prompt and permanent, as I can testify from personal experience." "If I wished to be satirical," continues Prof. Gross, "I should say that there are in our profession, as there are, indeed, in every other, two distinct classes of men, the thinking and the non-thinking. The former, whose number is exceedingly limited, accept every novelty, or great and sudden change, with suspicion, wisely concluding that the one ought not to be adopted until it has been fairly tested by well-conducted observation and experiment, and that the other should not be rejected without sufficient cause. The non-thinking man, on the contrary, eagerly lays hold of every novelty, and seldom stops to seek a reason for his new faith. He adopts it simply because his neighbor adopts it. Especially is this the case when the novelty, whatever it may be, has a distinguished parentage, as when it has received the sanction of a great name, or, perchance, if it had a transatlantic origin—Jones, Robinson, or Brown, in Europe, is always a greater man, indeed, far greater, than his namesake on this side of the water. The non-thinking man confounds progress with improvement. He does not weigh the pros and cons of a question; he takes a shorter route; sees things in a distorted light; assumes for granted things that he cannot comprehend; and jumps at conclusions. As the sheep follows the wether, so he follows his master, looks through his spectacles, believes in his infallibility, and swears by his authority. The more the assertion borders on the marvelous the more readily does he gulp it, so much easier is it to assume the truth of a proposition or statement, than to prove it by sound, logical argument, and inductive reasoning. I think I am not guilty of exaggeration in what I say. It really seems to me as if we were bereft of our senses. No sooner is a new remedy, a new operation, or a new method of treatment introduced to notice, than it is puffed into gigantic proportions, and invested with virtues as foreign to it as any other folly under heaven."¹

While this revered master and "Nestor of American Surgery" was thus urging the claims of blood-letting, and deplor-

¹ Dr. Gross, *op. cit.*, pp. 436-7.

ing it as a "lost art" on the floor of our national tribunal of the medical sciences, our distinguished colleague and valued friend, the late Dr. C. B. Nottingham, of Macon, was claiming our approval, before the Medical Association of Georgia, by an admirable essay in defence of a reasonable consideration for venesection as a remedy.

To the question, then, What is the essential condition of the general system and of the nerve-centres?—which can be said to constitute the pathologic basis of eclampsia, we have certainly found that neither recent experimental physiology, nor the deductions growing out of them, can give us a satisfactory and consistent answer when viewed in the light of clinical experience and the accepted principles of therapeutics. That congestion of the brain, that ureic poisoning of the blood, or that anæmia of the brain and its ganglia, can any one of them alone, or all of them combined, supply an answer to this momentous question—or that an answer at all can be given as to the profound and ultimate state which underlies the convulsive phenomena, are questions which, we fear, a later time and a more advanced method of investigation alone can answer. But there is left us one resource which, in the meantime, we may adopt, however humiliating the admission it involves;—however it may compromise and involve in doubt many of the deductions of experimental physiology, of experimental chemistry, of blood tests, of urine tests, and of other tests of other fluids and other secretions;—however inconclusive it may acknowledge vivisections and excitations of the brain, the nerve-centres and the nerves, whether they be mechanical, galvanic or toxæmic—can we acknowledge they have been in vain, but false lights that have deceived us? Perhaps not—*most surely not*—these investigations have taught us much; but certainly they have not taught us the pathology of ~~perpetual~~ convulsions.

hæmiperal

The humiliating resource left us is that we must retrace our steps, take up again and be contented with the interpretation of phenomena we were a little time ago contented with; and base upon it, with our eyes open and without groping, a pathology which will be consistent in accounting for the symptoms; and one too, which in every particular is confirmed by therapeutic results. To another age let us relinquish the ambitious enterprise of elucidating the cause of the causes that give rise

to puerperal eclampsia. We think we may be said to have both witnessed and realized its failure for the present. Let us drop back into more superficial interpretations, if so they be called, and be content again to generalize, along with the other forms of convulsive diseases, that over-studied, over-discussed, and over-wrought member of a common class, which we call eclampsia. Why not recognize for eclampsia that cause, than which we can recognize no other for any form of convulsions? The simple statement of this would be *irritation*; whether centric, whether peripheric, whether both in consensual relation. Will any one pretend to say that we have advanced so far that we must expunge the term and its significance as the common cause of every form of convulsive action? If so, let him look at the confusion its relinquishment has made in the study of eclampsia, and how such relinquishment has retarded the establishment of fixed views as to its pathology.

The one condition of irritation existing as the proximate cause of the convulsion will be ever recognized by its manifestations. Its recognition as the cause will go farther than any other theory to conciliate conflicting views, in regard to the concomitant states of the system, in regard to the phenomena in eclampsia, as analogous to those of dentition, epilepsy, tetanus and other forms of convulsions—and in regard to the results of every form of treatment which has been found most efficient in controlling the spasmodic action of this and every other kind. Whether there is congestion or anæmia of the brain and motor ganglia; whether some centric or peripheric irritation; whether the irritant exist in the form of urea or ammonia, or some zymotic poison, sophisticating the blood, and being carried by it to the motor and other centres; none will deny that any of these conditions existing in varying degrees are capable of furnishing the basis of eclampsia in the state of increased polarity, in which we have every reason to believe the brain and motor centres exist during pregnancy and labor. Should we attempt to examine the various causes we have above referred to as the instigators of the *irritation* (which some may say we superficially persist in recognizing as the *general* cause of convulsions,) and each one of which, as is well-known, has been made the characteristic of a distinct pathology—too much space would be occupied in the discussion. We might

say, however, that the selection of the relations of a single one would very nearly cover the entire class. Even such consideration, to be done with justice, would occupy much time. We refer to the relations of the blood and its circulation throughout the system, but more especially in the brain and motor ganglia, as influencing these instrumentalities in the evokement of eclamptic seizures.

There can be no doubt that any rapid or sudden changes in the quantity of blood circulating in the brain and ganglia, whether they be of diminution or increase, (unless to the degree of apoplectic oppression) are liable to give unsteadiness to muscular action, sometimes evoking convulsions.¹ Thus congestive apoplexy, in its inception, is most generally marked by convulsive action; inflammatory conditions of the centres or their membranes have often similar results; while not the less in syncope do we observe convulsive phenomena almost identical with those marking the beginning of apoplectic congestion. Thus, the changes in the volume of the circulation, either way, are constantly observed to supply the condition of *irritation* or *irritability*, which we consider necessary to the evokement of any kind of convulsive action whatever.

In addition to the changes above referred to relating to *quantity*, we know that the blood of the pregnant and the lying-in woman is often changed in *quality* by being filled with uneliminated materials, some of which are well calculated, even without alteration of volume, either way, to irritate the nerve-centres they may circulate in, and stimulate their activity, as strychnine would do—by sophisticating the blood on its way to the ganglia. *Irritation* then, in each one of these cases, however produced (be it the result either of modified quantity or modified quality of the blood supplying the ganglia) would, to say the very least, most probably give rise to convulsive phenomena.

¹ Some one has remarked that the blood, to the brain and nerve-centres, is, as it were, what the pendulum and weight are to the clock. It controls, steadies, and co-ordinates their action. Too heavy a pressure, like too long a pendulum, causes heaviness and slowness of action; while an inadequate supply, or its extreme diminution, like the removal of the weight—disconcerting entirely the action of the machine—renders irregular and uncontrollable all muscular activity throughout the entire system. There are doubtless such convulsions from such a cause; but from what we have seen they must be extremely rare, as accidents of the pregnant state.

The *eclamptic flush* is one of the most remarkable and characteristic indications to pregnant women of the approach of a convulsion. Turgid conditions of either the centric origin of the optic nerve or of its peripheral expansion, the retina, are the pathologic states in which subjective luminosity, whether in the form of sparks or flashes, have been found most often to occur in individuals other than those in the pregnant or puerperal condition. The circulation of the brain and that of the eye is so nearly a common one that the vascular condition of the retina, as determined by the ophthalmoscope, has become the criterion of cerebral vascularity. That luminous phenomena occur most frequently in persons of a full and plethoric habit would strongly favor the view that their constancy in puerperal eclampsia is due to a plethoric rather than to an anæmic condition of the brain.

Our own recent experience furnishes us with a case illustrative, in a remarkable degree, of the *reflex* character which subjective luminosity will sometimes assume. Rev. E. P. R., D.D., a learned and distinguished clergyman, had undergone ophthalmoscopic examination by Drs. H. D. Noyes and E. G. Janeway of New York. From his account, sub-retinal hemorrhage to a slight degree had been diagnosticated. He suffered no pain, the eye had a clear and healthy appearance, and vision, we think, was gradually improving. As his case progressed, however, he was constantly annoyed by luminous phenomena. These were especially troublesome at night after lying down in bed and when in a dark room. They were invariably evoked by *sounds*, whether near or distant, the ticking or striking of the mantle clock, and also the striking of the city clock, perhaps miles away, would be, as long as he was awake, invariably responded to, *each stroke by a decided flash*, in the affected eye!

There can be no doubt that here, the turgid condition of the retina, consequent upon the minute effusion or upon some limited congestion, had exalted its responsive excitability so that by the auditory nerve and by every other avenue of afferent excitation this increased polarity was made to manifest itself in luminous *discharges*. Thus do we interpret this ease of simple reflex "eclampsia" restricted to and extending no further than the retina of an injured eye.

In applying the phenomena of this remarkable case to the il-

lustration of the flashes which form a part of that more complex tableau, true puerperal eclampsia, may we not legitimately predicate that the polarity of both the brain and retina of the plethoric pregnant woman is exalted by the over-turgid condition of their common circulation; and that when a convulsion is excited by any external or internal provocative, as blood-poison, or a labor pain, it is initiated first by a discharge from the sensory ganglia, in the form of *flashes*, as the sound of the bell touched off the explosion in the case above described; and then that, in the same manner, came the discharges from the *motor batteries* at the base of the brain, and from the medulla oblongata, in the form of spasmodic muscular jactitations with loss of consciousness, the result of which last discharge we call convulsions? In answer to the above question we can only say that in addition to the force of argument to be drawn from analogy, therapeutics and experience seem both most strongly to justify our view: In the case of a pregnant woman, whose general fulness and cerebral disturbances had caused us much uneasiness, even up to the eighth month, we were told that during the morning "she had been complaining of flashes." Just as we entered the room, she exclaimed, "I am going blind!" She had torturing pains in her head, her face was flushed, and her speech slow, somewhat indistinct and difficult. We administered immediately forty grains of bromide of potassium; and, remaining with her, in less than an hour gave her thirty grains more. A third dose of thirty grains was given in a few hours. Her symptoms began to recede after the first dose, and during the evening they entirely disappeared. Three grains of quinine, alternated every two hours by twenty grains of bromide of potassium to the extent of—quinine, grs. xii., and bromide of potass., grs. lx., were given each day for the next two days. She, shortly after this, experienced the most profuse, and to her, alarming diuresis. Her swollen legs, and hands, and face shrunk in a remarkable manner. She experienced no return of her unpleasant symptoms; continued with 3 grs. of quinine and \mathfrak{D} i. bromide potass. every day till after the time of her labor at full time, which took place nearly a month afterwards. She was happily delivered of a healthy living child, without having experienced a single eclamptic or any other unpleasant symptom.

Another lady, in her seventh month of gestation, was very

plethoric, apparently; but, we believe, was also very anæmic—overfulness of watery blood. She suffered from dysuria, had vesical tenesmus; urine scanty, it was not tested. She was œdematous in face, hands and legs; she complained of “a pain like an iron band” around her head, which came on in the evening. She bore quinine badly from idiosyncrasy; the little we could give did not modify the pain. Bromide of potassium failed to correct any of the symptoms. Being called, under the apprehension that premature labor had begun, she said that “each time she had a pain there was a sensation of light.” Her extreme pallor and her feeble pulse disinclined us to resort to bleeding. “Flashes,” however, had been our signal for the most active treatment for over thirty years. Active treatment most always meant blood-letting, previous to the era of morphine, chloral, and the bromides. We now, however, with reluctance, prepared to bleed in the most careful and limited manner. An 8 oz. glass tumbler was placed in the basin to catch and measure the blood. Not more than a gill of blood had been drawn when the pain ceased and the sensations of light became less frequent. By the time the tumbler was nearly full, she stated that she was “entirely relieved.” She was somewhat faint in walking back to the bed, but was perfectly comfortable for many hours. The excretion of urine was without pain and the flow more abundant; but labor came on in about eight hours, and she was delivered naturally of a still-born premature infant. Neither during nor after labor did she experience any symptoms approaching those of eclampsia.

Twenty years ago, we believe, we would have bled this lady perhaps a week earlier; we also believe that bleeding at that period would have prevented the miscarriage and preserved the life of the fœtus; and we further believe that it died of uræmic intoxication, *which the diuretic effect of a moderate bleeding* would have eliminated from the blood of the mother. As it was, the bleeding was made early enough to stay convulsions in the mother, but not in time to prevent the fatal poisoning of the child. We verily believe we may sometimes know too much for the welfare of our patients! We must make haste to unlearn some of our modern pathology, or retain it only for ornament.

The above will at the present time be regarded by some as rather a striking case; but in times not very long past, when

bleeding was a more common, indeed almost the only remedy, such cases, with the exception of the small amount of bleeding required, were quite familiar to many.

But it may be asked, why in the pregnant condition particularly should we find this irritability, or predicate this reflex responsiveness of the motor ganglia to both internal impressions through the blood, as well as to external impressions through any of the sensory nerves? The cause of this condition we have used a term every day becoming less familiar to designate: "Increased polarity of the nerve-centres," if stated, would we believe be perfectly comprehensible; but, being none the less theoretical on that account, it is inadmissible except as an hypothesis. In the pregnant condition, the demand upon the woman's resources of nutritive supply are greatly increased. That she is, in certain particulars, temporarily endowed with a nervous apparatus correspondingly enhanced in power to answer such demand, the investigations of Dr. Robert Lee, of London, establishing the rationale of the increased growth and muscular power of the uterus, most unanswerably demonstrate. Ganglia of the womb, before undiscoverable, became rapidly developed both in size and activity to a degree commensurate with the control and with the activities they are to give to the enormously-developed vascular, muscular and nutrient endowments of the gravid uterus. It is known that there are some other portions of the pregnant woman's animal economy wakened into an analogous exaggeration of temporary growth: the spleen becomes enlarged; the heart generally acts with an increased force; there is general plethora, although the blood may be thin; and ordinarily the adipose and connective tissue are obviously increased. With all this before us—the greatly enhanced nutrition everywhere, with this plainly indicated teleological plan by which increased nutrition and power, as in the womb, are made to rest upon a temporarily organized apparatus of nerve force, why may we not legitimately predicate an analogously increased nutrition and normally increased polarity and activity in the cerebral and basal ganglia of the encephalon, in order that resources of nerve-force may be engendered in amounts adequate to the sensory, the motor, and the nutrient supply which the general system is, during that time, called upon to yield? Such, in the want of demonstrated proof, we can only say we believe is necessarily the actual and normal condition of

augmented nervous apparatus for the production of an amount of nerve-force adequate to the augmented demand. This may be said to render the brain of the pregnant woman, and all the ganglia pertaining to her encephalon, so many highly polarized magazines of potential force, ever liable under certain circumstances to become actual and uncontrollable, whenever it is awakened into action by any of the imminent conditions of quality or quantity of the blood which we have described. Any sudden increase or diminution of blood—even though normal in its constituents—in the structure of these ganglia; the circulation in them of ureic or ammoniacal blood; perhaps the coming to them of zymotoxics or any other accidental or occasional irritant; would any of them be fully sufficient—as it is indeed claimed for each in its turn, by etiologists of various special tenets—to touch off the eclamptic explosion, by centric irritation. And no less, as is sufficiently familiar to all, does *reflected* irritation, from any distant organ, set in train the identical role of phenomena,—the womb oftenest of all organs;—hence the expression of the philosophic and observing Dr. Power, “metastatic labor-pains,” which being interpreted by us, means *reflected and distributed uterine irritation*.

Arriving at this point in our examination of the subject and standing in the end upon ground, as it may be said, so little advanced beyond the status of a period now drifted into the past, we contemplate with hesitation, almost with regret, the vast field from which we have receded, and which we earnestly ask practitioners for a while yet to relinquish. As we return, for the sake of more certainty, to the simple and ever consistent doctrine of irritation, and then compare it with the position to which we had progressed in our misdirected study of eclampsia, we can estimate what a long and dubious journey we have made, very nearly in vain, so far as its true pathology has been advanced, or its therapeutics, as it could legitimately be based upon the present pathology. It is like a new country, or unknown land, into which the pioneers, the sappers and the miners, had pushed forward, in which they had made valuable explorations, had learned much, and increased the knowledge of the world—but yet a territory which permanently could not be held. The older pathology based upon cerebral plethora, the newer one resting upon the undoubted existence of uræmia, and still farther the most recent of all, that of cerebral anæmia—are, each

one of them, too incomprehensive to embrace all the facts and phenomena, therapeutic as well as pathologic, of puerperal convulsions.

While then these views and lines of investigation have added immensely to our knowledge of the concomitant conditions of the pregnant woman, in which eclampsia may originate; while, under some strange misconception, new and most valuable remedies have been added to our successful means of treatment during the reign of this doctrine, the very efficiency of whose remedial influence *subverts the doctrine*; while even the death-rate, through the instrumentality of these means, has been greatly reduced; and while the results of these very investigations strongly illustrate and confirm the true pathology; still it has gradually been every year becoming more and more apparent, that no one of these doctrines, nor all of them combined, can ever replace that more consistent, though less recon-dite pathology, claimed in *centric and peripheral irritation*.

In nearing the close of this discussion, we may state that some very high authorities still look no further than this even at the present day, for the pathology of eclampsia. Dr. Robert Barnes, of London, very moderately discusses blood-letting as a measure to be considered in the treatment of puerperal convulsions. Recognizing the inadequacy apparently, of some of the more modern views, he, in the plainest language contents himself with centric and peripheral irritation as the proximate cause of the attack. He advocates "measures for moderating the excess of central irritability as almost always useful. The most available of these," he says, "is the induction of anæsthesia by chloroform." Belladonna, the bromides and chloral are given the highest praise; while he proposes, as a still more rapid method of inducing anæsthesia to administer sometimes, in the future, the nitrite of amyl. We are much surprised to see that he then, almost reluctantly, approaches the subject of blood-letting. "We cannot yet," says he, "discuss the treatment of eclampsia without referring to the practice of bleeding. To advocate anæsthesia is practically to condemn venesection. * * * I am one of those who think there is more of fashion than of wisdom in the almost absolute oblivion of the lancet. But in this particular case, I do not regret the disuse into which it is falling. It is very easy to tell of cases in which bleeding has been followed by recovery, and of other cases in

which other treatment has been followed by death. I believe I have seen distinct relief ensue upon moderate abstraction of blood from the arm, or by the application of leeches to the temples. And where there is distinct evidence of plethora with marked engorgement of the vessels of the face, it is judicious, I think, to apply eight or twelve leeches to the temples, but not to the exclusion of anæsthesia. In delicate women with a feeble circulation bleeding in any form should be rigorously condemned. And we must not forget that the process of labor is usually attended by a loss of blood quite as great as is good for the patient.”¹ Such measured admission of the occasional possible benefits of venesection—if indeed we can recognize it as an admission at all—in one of Dr. Barnes’ wide experience and superior judgment, we must admit, surprises and disappoints us. In the American edition of his invaluable work on Obstetric Operations, the American editor, Dr. B. F. Dawson, has added greatly to the value of the work, as we have it, by the introduction of an abstract of the views of Prof. Fordyce Barker from a former number of this journal;² and blood-letting is here forcibly and most judiciously advocated. After speaking with great discrimination of its value in general and cerebral congestion, in uterine and renal turgescence, Dr. Barker uses the following language, which to many must call up reminiscences of satisfactory results in their own experience. “It has seemed to me there is some liability to err in the neglect of blood-letting from the feeling that this measure should never be resorted to unless the patient is in a sthenic condition. But some of the most striking instances of its usefulness have occurred under my observation when the patient was extremely anæmic.”

Previous to the discovery of those powerful cerebral exsanguinators now known to us in anæsthetics, and to their application in eclampsia, we were in the habit of advising, in clinical lectures, that, “though some few cases of eclampsia may even be materially injured by bleeding, yet this minority is so small a one as compared with the vast majority who would be greatly benefited by it, I would therefore advise you, in default of any experience of your own, to be guided by that of others, and re-

¹ Abstract from Lumleian Lectures in *Brathwaite's Retrospect*. Part lxviii. • January 1874; p. 222.

² *American Journal of Obstetrics, etc.* Vol. iii. No. 3. 1871.

gard every case of puerperal convulsions in an ordinarily plethoric woman as a case for some blood-letting. Take from 8 ozs. to 16 ozs. from every one such as above described. If, on observing the pulse and other symptoms you find benefit rather than injury, take more, and even to 30 ozs., should the convulsion continue." We cannot say that even now we are prepared to modify very materially the above precept. Empirical as it may appear to some, it certainly seems to us much safer than the equally empirical rule sought to be enforced by a large number of modern teachers, that of *not bleeding at all*.

Prof. Joseph A. Eve, of this city, after an experience of nearly fifty years in the active practice and conscientious teaching of obstetrics, we are gratified to add, is in full accord with our own general precepts relating to blood-letting. This much honored and distinguished gentleman, to whose transcendent worth, both as a physician and a man, we have ventured to dedicate this unworthy contribution, though always accurately and profoundly familiar with every phase of both the pathology and therapeutics in this subject, and though remarkable for his deferential consideration of the opinions of others, has yet never at any time receded in practice from the high importance which, in the beginning of his long and useful career, he attached to blood-letting. Valuable instructions, based on large and intelligent experiences, too often perish and are lost to the profession with the sage who gathered them. Such as his most certainly should have a wider distribution and more permanent record than the desk or the lecture-room. For this reason do we epitomize and ask their publication with our own in this widely circulating journal of American obstetric experience.

We feel inclined to suggest in this place, hopelessly as it may appear to some: first, that the occurrence of eclampsia in a pregnant woman will probably at no distant day be again regarded as *prima facie* evidence, not of an anæmic brain, but that that woman is the subject of *cerebral irritation*; either primary, depending upon *irritative centric hyperæmia*; or secondary, depending upon *obstructive renal congestion*, either or both of which conditions might have been averted, and of course with them, the consequent eclampsia, by a timely moderate blood-letting, had their existence been recognized and their dangerous influence duly estimated at the proper time.

Secondly, that while we will not now distinctly advise that every eclamptic woman should be bled, we must unhesitatingly decide, in view of the almost uniform turgescence, either in the brain or in the kidney—this latter not promptly amenable to any other measure of treatment—that the omission of venesection from the treatment of eclampsia should ever be regarded rather as the well-considered and reluctantly admitted *exception*, than as the rule of practice.

CONCLUSIONS.

From the foregoing discussion, we think it will be admitted, that neither the recent investigations of experimental physiology, nor clinical observation, nor the results of therapeutics—taking any class of them singly, or all of them combined—can authorize us, at the present time, in recognizing either cerebral plethora or cerebral anæmia, or yet uræmia, or other toxic condition of the blood, as furnishing a uniform etiology for puerperal eclampsia. For, though any one or all of these conditions of the blood and blood-vessels may exist, and underlie and strengthen the true proximate cause, they should, all of them, for the present, be regarded but as influences, very frequently present, and when present, more or less tending to excite eclamptic perturbation; but that no particular one of them is specially necessary as an element in its production.

2. That the pathology and proximate cause of puerperal eclampsia cannot be safely assigned, in the present advanced though inconclusive stage of scientific investigation, as being even yet legitimately removed beyond that one, which may be generalized in its proximate etiological relation, and assignable to *every form of convulsive action*—the tetanic, the hysteric, the infantile, the epileptic, or the anomalous; viz., *nervous irritation*. This irritation may be either centric or peripheric, or both combined;—that is, an exalted excitability of the reflex excito-motory instrumentalities, as they exist in receptive sensory nerves and surfaces, or in the motor or responsive ganglia, and in the motor nerves. For example, a distant excitor nerve—as in the uterus, or it may be a dental filament of the fifth pair—being impressed, awakens such molecular change in abnormally polarized motor cerebral, or motor spinal centres, as to evoke general convulsive action;—in the one case, the infantile convulsions of dentition may occur, in the other, the

hysteric or eclamptic. Still further change the receptive excitor to a traumatic injury, and a tetanic convulsion may take place—the characterization of each being dependent on the determining concomitants.

3. That though this pathology, and the designation of *irritation* as the proximate cause of eclampsia, *and of all convulsions*, will be familiar to many as the manifest truism of a period almost past, it may well be recognized on that very account—seemingly paradoxical as it is—as a real and valuable advance; inasmuch as to accept it, will be to relinquish the conjectural, the uncertain, and the unknown, for the familiar, the well-established, the consistent and the safe. We make a real advance when we go back to take up and incorporate an undoubted fact, which we had forgotten or neglected or passed by, and which is one of the indispensable elements in the development of truth. If such pathology be but a truism, we must then be satisfied with truisms, until less inconclusive investigation ripens into consistent truth, the researches that have been recently so prematurely credited as our guides.

4. That the proximate cause of eclampsia having been recognized as one and the same, as that common cause of so many—of almost all morbid nervous phenomena—some controllable, some intractable, viz., centric and peripheral irritation, or exaggeration of reflex excitability—then the sole, the grand and consummate indication unavoidably takes precedence, as the precept and the object of all treatment—to *quiet and to subdue irritation*. The prophecy of diagnosis is here confirmed by the history of cure. The surmises of etiology are endorsed by the consistent results of therapeutics.

5. That irritation being recognized as the proximate cause of the frightful phenomena we have to combat in puerperal eclampsia, the well-known and universally acknowledged superiority and efficiency of opium and its preparations for controlling irritation, renders it the first, the most ready, and the most promptly effectual of all the means at the command of the practitioner. That its hypodermic administration, either alone or in combination with atropia, and its ready applicability by the rectum, still further entitle opium to unrivalled pre-eminence as the combatter and controller of, and the first application to be made in, eclamptic irritation of the motor ganglia and excitor nerves.

6. That next to opium, in its general applicability, and superior to opium in many specific cases—utterly indispensable in some—we should rank blood-letting as *the* sedative, upon which we may most confidently rely for direct and immediate influence in overcoming most of the forms of centric irritation and for subduing convulsive action—especially as it is found in the eclampsia of pregnancy and childbed.

7. That the statistics showing the death-rate in puerperal eclampsia—under treatment by bleeding on the one hand, and by anæsthetics on the other—while they go largely to exhibit the great value of these latter agents as the subduers of centric irritation, and to show that their *combined* power has been able to effect in modern times better results than venesection *alone*, or almost entirely unaided in the past;—still, these very statistics—even showing as they do, in some sense, an unfavorable comparison—go far to contradict the anæmic pathology of some modern authors, and to convince us, that venesection, so far from having been injurious, was, in this disease, a largely beneficial method of treatment.

8. That in the light of more recent investigations, a careful review of blood-letting as practised in the past, so far from resulting in its condemnation, confirms the value of venesection as a method of promptly sedating nearly all the causes of irritation upon which eclampsia depends. That while modern discovery has supplied to the treatment of eclampsia many valuable agents that control convulsions, independently of venesection; the very success of these remedies, while it plainly contradicts the pathological views upon which the condemnation of venesection is founded, *should strongly re-establish our confidence in blood-letting*; as such success leaves it highly probable that it is by virtue of their being *cerebral exsanguinators*—by their lessening the quantity of blood in the brain, and by preventing “the determination of blood to the head”—that they, like bleeding, have been found to be most valuable remedies in puerperal convulsions.

9. That though hereafter—since we are in the possession of many valuable remedies of like action, and capable, by a similar *modus operandi*, of subduing eclamptic irritation, both centric and peripheral—the resort to blood-letting may not be so often necessary as formerly it was, as an impromptu and immediate direct sedative, venesection should yet by all means

be retained among the most reliable of all our reliances. That even when a convulsion has been relieved by other means, or when the danger of an impending convulsion has apparently gone by, no woman, of plethoric habit especially, should be discharged by the medical attendant, until after he has carefully considered—in view of the tendency manifested, and the frightful danger just escaped—whether or not this should be one of the measures of that prophylactic after-treatment, so invariably the care and the anxiety of the thoughtful and faithful physician in charge of such a pregnant woman.

10. That venesection, when discussed in its relations to eclampsia, has been, and is now still held, too strictly to the consideration of its influence upon the encephalon and upon the vascular condition of the ganglia within the cranium; and upon these alone. That, though cerebral plethora may be denied by some, there can be no doubt of *renal turgescence*, in many cases—modifying the circulation and action of the kidney—causing albuminuria, and preventing the elimination of *urea*, which being retained and circulated in the blood, is often doubtless one of the causes of eclamptic seizure. Therefore, that venesection should in this relation be carefully and hopefully deliberated upon with reference to the “temporary,” but none the less, for the time, devastating Bright’s disease of pregnancy. That by the same *rationale* by which, among the older practitioners, bleeding became incidentally a diuretic in dropsy, so, in an analogous condition of these oppressed and crippled organs, may it become a rational method of restoring diuresis to eliminate urea and other irritating products from the blood.

And lastly, in regard to the bromides, and to chloroform, and to chloral, and to quinine, and to applications of ice and cold affusions, and in regard, as well, to a large number of other remedies—some, like those first mentioned, being agents considered of the highest value—one common *therapeutic endowment* seems, in varying degrees, to be possessed by them all—that they are all, like venesection, nervous sedatives, *the subduers of nervous irritation*;—and, that falling into singular coincidence, by general consent, their therapeutic action, like that of venesection, is accompanied by one common physical result—that of lessening the amount of blood in the brain.

